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and the periodicity of certain phases. We can not, however, draw a hard-and-fast line between the two categories. The factor controlling a varietal characteristic often produces effects in different parts of the plant. For example, the factors which lead to the production of a colored flower no doubt also in certain cases cause the tinging seen in the vegetative organs, and affect the color of the seed. Heribert-Nilsson suggests that fertility between species is a matter of close similarity in genotypic (factorial) constitution rather than of outward morphological resemblance. Forms sundered by the systematist on the ground of gross differences in certain anatomical features may prove to be more nearly related because the differentiating factors happen to control less conspicuous features.

THE SUPPLY OF OXYGEN TO THE TISSUES

By J. R. BARCROFT

PRESIDENT OF THE PHYSIOLOGICAL SECTION

PROMINENT among the pathological conditions which claimed attention during the war was that of insufficient oxygen supply to the tissues, or anoxæmia. For this there were several reasons; on the one hand, anoxæmia clearly was a factor to be considered in the elucidation of such conditions as are induced by gas poisoning, shock, etc. On the other hand, knowledge had just reached the point at which it was possible to discuss anoxæmia on a new level. It is not my object in the present address to give any account of war-physiology—the war has passed, and I, for one, have no wish to revive its memories, but anoxæmia remains, and, as it is a factor scarcely less important in peace than in war pathology, I think I shall not do wrong in devoting an hour to its consideration.

The object of my address, therefore, will be to inquire, and, if possible, to state, where we stand; to sift, if I can, the knowledge from the half-knowledge; to separate what is ascertained as the result of unimpeachable experiment from what is but guessed on the most likely hypothesis. In war it was often necessary to act on defective information, because action was necessary and defective information was the best that was to be had. In this, as in many other fields of knowledge, the whole subject should be reviewed, the hypotheses tested experimentally, and the gaps filled in. The sentence which lives in my mind as embodying the problem of anoxæmia comes from the

pen of one who has given more concentrated thought to the subject than perhaps any other worker—Dr. J. S. Haldane. It runs, “Anoxæmia not only stops the machine, but wrecks the machinery.” This phrase puts the matter so clearly that I shall commence by an inquiry as to the limits within which it is true.

Anything like complete anoxæmia stops the machine with almost incredible rapidity. It is true that the breath can be held for a considerable time, but it must be borne in mind that the lungs have a volume of about three liters at any moment, that they normally contain about half a liter of oxygen, and that this will suffice for the body at rest for upwards of two minutes. But get rid of the residual oxygen from the lungs only to the very imperfect extent which is possible by the breathing of some neutral gas, such as nitrogen, and you will find that only with difficulty will you endure half a minute. Yet even such a test gives no real picture of the impotence of the machine—which is the brain—to “carry on” in the absence of oxygen. For, on the one hand, nearly a quarter of a minute elapses before the reduced blood gets to the capillary in the brain, so that the machine has only carried on for the remaining quarter of a minute; on the other hand, the arterial blood under such circumstances is far from being completely reduced—in fact, it has very much the composition of ordinary venous blood, which means that it contains about half its usual quatum of oxygen. It seems doubtful whether complete absence of oxygen would not bring the brain to an instantaneous standstill. So convincing are the experimental facts to any one who has tested them for himself, that I will not further labor the power of anoxæmia to stop the machine. I will, however, say a word about the assumption which I have made that the machine in this connection is the brain.

It can not be stated too clearly that anoxæmia in the last resort must affect every organ of the body directly. Stoppage of the oxygen supply is known, for instance, to bring the perfused heart to a standstill, to cause a cessation of the flow of urine, to produce muscular fatigue, and at last immobility, but from our present standpoint these effects of anoxæmia seem to me to be out of the picture, because the brain is so much the most sensitive to oxygen want. Therefore, if the problem is the stoppage of the machine due to an insufficient general supply of oxygen, I have little doubt that the machine stops because the brain stops, and here at once I am faced with the question how far is this assumption and how far is it proven fact? I freely answer that research in this field is urgent; at present there is

too great an element of assumption, but there is also a certain amount of fact.

To what extent does acute anoxæmia in a healthy subject wreck the machinery as well as stop the machine? By acute anoxæmia I mean complete or almost complete deprivation of oxygen which, in the matter of time, is too short to prove fatal. It is not easy to obtain quite clear-cut experiments in answer to the above question. No doubt many data might be quoted of men who have recovered from drowning, etc. Such data are complicated by the fact that anoxæmia has only been a factor in their condition; other factors, such as accumulation of carbonic acid, may also have contributed to it. The remarkable fact about most of them, however, is the slightness of the injury which the machine has suffered. These data, therefore, have a value in so far as they show that a very great degree of anoxæmia, if acute and of short duration, may be experienced with but little wreckage to the machine. They have but little value in showing that such wreckage is due to the anoxæmia, because the anoxæmia has not been the sole disturbance.

It is rather fashionable at present to say that "the whole question of acidosis and anoxæmia is in a hopeless muddle." To this I answer that, if it is in a muddle, I believe the reason to be largely because schools of thought have rallied round words and have taken sides under the impression that they have no common ground. The "muddle," in so far as it exists, is not, I think, by any means hopeless; but I grant freely enough that we are rather at the commencement than at the end of the subject, and that much thought and much research must be given, firstly, in getting accurate data, and, secondly, on relating cause and effect, before the whole subject will seem simple. No effort should be spared to replace indirect by direct measurements. My own inference with regard to changes of the reaction of the blood, based on interpretations of the dissociation curve, should be checked by actual hydrogen ion measurements, as has been done by Hasselbach and is being done by Donegan and Parsons. Meakins also is, I think, doing great work by actually testing the assumptions made by Haldane and himself as regards the oxygen in arterial blood.

For the anoxic type of anoxæmia two forms of compensation at once suggest themselves. The one is increased hemoglobin in the blood; the other is increased blood-flow through the tissues. Let us, along the lines of the calculations already made, endeavor to ascertain how far these two types of compensation

will really help. To go back to the extreme anoxic case already cited, in which the hemoglobin was 66 per cent. saturated, let us, firstly, see what can be accomplished by an increase of the hemoglobin value of the blood. Such an increase takes place, of course, at high altitudes. Let us suppose that the increase is on the same grand scale as the anoxæmia, and that it is sufficient to restore the actual quantity of oxygen in one c.c. of blood to the normal. This, of course, means a rise in the hemoglobin value of the blood from 100 to 150 on the Gowers' scale. Yet even so great an increase in the hemoglobin will only increase the oxygen taken up in the capillary from each c.c. of blood from .031 to .036 c.c., and will therefore leave it far short of the .06 c.c. which every cubic centimeter of normal blood was giving to the tissue. So much, then, for increased hemoglobin. It gives a little, but only a little, respite. Let us turn, therefore, to increased blood-flow.

In the stagnant type of anoxæmia the principal change which is seen to take place is an increase in the quantity of hemoglobin per cubic millimeter of blood.

This increase is secondary to a loss of water in the tissues, the result in some cases, as appears from the work of Dale, Richards, and Laidlaw, of a formation of histamine in the tissues. Whether this increase of hemoglobin is to be regarded as merely an accidental occurrence or as a compensation is difficult to decide at present. Roughton's calculations rather surprised us by indicating that increased hemoglobin acted less efficiently as a compensatory mechanism than we had expected. This conclusion may have been due to the inaccuracy of our assumptions. I must therefore remind you that much experimental evidence is required before the assumptions which are made above are anything but assumptions. But, so far as the evidence available at the present time can teach any lesson, that lesson is this: The only way of dealing satisfactorily with the anoxic type of anoxæmia is to abolish it by in some way supplying the blood with oxygen at a pressure sufficient to saturate it to the normal level.

It has been maintained strenuously by the Oxford school of physiologists that Nature actually did this; that when the partial pressure in the air-cells of the lung was low the cellular covering of that organ could clutch at the oxygen and force it into the blood at an unnatural pressure, creating a sort of forced draught. This theory, as a theory, has much to recommend it. I am sorry to say, however, that I can not agree with it on the present evidence. I will only make a passing allusion to the

experiment which I performed in order to test the theory, living for six days in a glass respiration-chamber in which the partial pressure of oxygen was gradually reduced until it was at its lowest—about 45 mm. Such a pressure, if the lung was incapable of creating what I have termed a forced draught, would mean an oxygen pressure of 38.40 mm. of mercury in the blood, a change sufficient to make the arterial blood quite dark in color, whereas did any considerable forced draught exist, the blood in the arteries would be quite bright in color. Could we but see the blood in the arteries, its appearance alone would almost give the answer as to whether or no oxygen was forced, or, in technical language, secreted, through the lung wall. And, of course, we could see the blood in the arteries by the simple process of cutting one of them open and shedding a little into a closed glass tube. To the surgeon this is not a difficult matter, and it was, of course, done. The event showed that the blood was dark, and the most careful analyses failed to discover any evidence that the body can force oxygen into the blood in order to compensate for a deficiency of that gas in the air.

Yet the body is not quite powerless. It can, by breathing more deeply, by increasing the ventilation of the lungs, bring the pressure of oxygen in the air cells closer to that in the atmosphere breathed than would otherwise be the case. I said just now that the oxygen in my lungs dropped to a minimal pressure of 45 mm.; but it did not remain at that level. When I bestirred myself a little it rose, as the result of increased ventilation of the lung, to 56 mm., and at one time, when I was breathing through valves, it reached 68 mm. Nature will do something, but what Nature does not do should be done by artifice. Exploration of the condition of the arterial blood is only in its infancy, yet many cases have been recorded in which in illness the arterial blood has lacked oxygen as much as or more than my own did in the respiration chamber when I was lying on the last day, with occasional vomiting, racked with headache, and at times able to see clearly only as an effort of concentration. A sick man, if his blood is as anoxic as mine was, can not be expected to fare better as the result, and so he may be expected to have all my troubles in addition to the graver ones which are, perhaps, attributable to some toxic cause. Can he be spared the anoxæmia? The result of our calculations, so far, points to the fact that the efficient way of combating the anoxic condition is to give oxygen. During the war it was given with success in the field in cases of gas-poisoning, and also special wards were formed on a small scale in this country in which the level

of oxygen in the atmosphere was kept up to about 40 per cent., with great benefit to a large percentage of the cases. The practise then inaugurated is being tested at Guy's Hospital by Dr. Hunt, who administered the treatment during the war.

Nor are the advantages of oxygen respiration confined to pathological cases. One of the most direct victims of anoxic anoxæmia is the airman who flies at great heights. Everything in this paper tends to show that to counteract the loss of oxygen which he sustains at high altitudes there is but one policy, namely, to provide him with an oxygen equipment which is at once as light and as efficient as possible—a consummation for which Haldane has striven unremittingly. And here I come to the personal note on which I should like to conclude. In the pages which I have read views have been expressed which differ from those which he holds in matters of detail—perhaps in matters of important detail. But Haldane's teaching transcends mere detail. He has always taught that the physiology of to-day is the medicine of to-morrow. The more gladly, therefore, do I take this opportunity of saying how much I think medicine owes and will owe, to the inspiration of Haldane's teaching.

INTENSIVE CULTIVATION

By Professor FREDERICK KEEBLE

PRESIDENT OF THE AGRICULTURAL SECTION

THERE is, so far as I can discover, no reason—save one—why I should have been called upon to assume the presidency of the Agricultural Section of the British Association, or why I should have been temerarious enough to accept so high an honor and such a heavy load of responsibility. For upon the theme of Agriculture as commonly understood I could speak, were I to speak at all, but as a scribe and not as one in authority. The one reason, however, which must have directed the makers of presidents in their present choice is, I believe, so cogent that despite my otherwise unworthiness I dared not refuse the invitation. It is that, in appointing me, agriculturists desired to indicate the brotherhood which they feel with intensive cultivators. As properly proud sisters of an improved tale they have themselves issued an invitation to the Horticultural Cinderella to attend their party, and in conformity with present custom, which requires each lady to bring her partner, I am here as her friend.